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EXPERIMENTAL PRECOGNITION AND ITS IMPLICATIONS

BY ROBERT H. THOULESS, M.A., PH.D.

EXPERIMENTS on precognition were being carried out in Rhine's Parapsychology Laboratory at Duke University in 1933 and have been continued since then.¹ The method now generally used is one in which the subject guesses the order of symbols in a future arrangement of a pack of Zener cards. After the guesses, the pack is shuffled and then cut at a randomly determined point. The point of the cut may be determined by the use of dice whose falls indicate the position in a table of random numbers at which is to be found the number indicating the point of cut. The importance of this cut is that it would reduce the mean chance expectation of success to five even if the subject could have complete knowledge of the order of the pack after shuffling. It thus overcomes any objection to the procedure on the grounds either that the subject could have normal or paranormal knowledge of the original order and of the effect of shuffling, or that the experimenter could influence the final order of the cards in shuffling. Significant successes have been obtained under these conditions.

Further evidence of the possibility of precognition under experimental conditions was provided by Soal's experiments on Mr Shackleton in which the subject consistently guessed one card ahead in card guessing experiments.² Although in this form the experiments might be explained as contemporary clairvoyance without precognition, this explanation was eliminated in the form of the experiment in which the next exposure was not yet determined at the moment of guessing, being subsequently determined by an activity of Mrs Goldney. At first sight, there seems to be an important difference between these experiments and the

¹ J. B. Rhine, 'Evidence of Precognition in the Co-variation of Salience Ratios', *Journal of Parapsychology*, vol. 6, no. 2 (June 1942), pp. 111-43.

² S. G. Soal and K. M. Goldney, 'Experiments in Precognitive Telepathy', *Proc. S.P.R.*, vol. 47, pt. 167 (1943), pp. 21-150.

precognition experiments in the Duke Laboratory, since in the Soal-Goldney experiment the intention of the subject was to guess the card turned up and not the next one on which the hit was actually scored. Dr Soal tells me, however, that this difference only applied to the initial experiments; after Shackleton knew that he was succeeding on the next card, his intention was to succeed on that card. As in the more usual form of precognition experiment, the intention of the subject was directed toward correspondence with a future event.

During the last twelve months I have experimented on myself using the Duke Laboratory technique described above.¹ In 14 runs through the Zener pack, I obtained an excess of successes over mean chance expectation with odds of 50 to 1 against occurrence by chance. In an earlier experiment on card guessing with my wife as agent, I found a tendency to +2 displacement with odds against chance of 500 to 1.²

THE WORD 'PRECOGNITION'

Like many of the terms which we have inherited in parapsychology, the word 'precognition' is somewhat misleading as applied to these experimental results. There is no doubt about the appropriateness of the prefix 'pre', but I do not feel equally happy about the use of the term 'cognition'. 'Cognition' seems to imply not only that the subject's intention is that his guesses should correspond to some future event but also that he should be generally right and that he should know that he is right. The last two of these conditions have not been fulfilled in any of the experiments described above. On the other hand, precognitions in this sense have been described in the world outside the laboratory. It does not matter for our present purpose whether such reports are well-attested or not: we are concerned now with a question of linguistics not of fact. A prophet might say, for example: 'You will meet with a misfortune five days from now.' If he is generally right in such predictions, and if he has the same sort of assurance that he is right as in a normal prediction by rational inference, then we should be inclined to say that he knows (or cognises) the future. It would, however, be doing some violence to the ordinary use of language if we said that he knew the future when all that we meant was that there was a statistically

¹ R. H. Thouless, 'A Comparative Study of Performance in Three Psi Tasks', *Journal of Parapsychology*, vol. 13, no. 4 (December 1949), pp. 263-73.

² R. H. Thouless, 'Experiments on Paranormal Guessing', *British Journal of Psychology*, vol. 33, pt. 1 (July 1942), pp. 15-27.

significant correspondence between some of his prophecies and some of the events that later took place.

Before suggesting an alternative term for 'precognition', I should like to consider the relationship between paranormal precognition and some facts of normal psychology. There is a curious habit of talking about the foretelling of the future as if this were essentially a paranormal activity. In truth, what is odd about experiments in paranormal precognition is not the fact that they show results in which a human activity is determined by a future event, but that this takes place without the operation of any normal, rational grounds on which the future event can be inferred. In normal life, we are continually foretelling the future ; there is nothing odd about that. I receive by post a notice telling me that a meeting will take place at a certain hour on a certain day. The senders of that notice were foretelling the future no less than a gipsy who told me that I would live till I was seventy-five. Of course, there is a difference between the two cases. Normal foretelling of the future is that in which we either use rational inference from past or present events and known causal connections between these and the predicted future events, or else that in which we use the same grammatical form of words to state some future fact depending on human volition, e.g. 'I will catch the 3.25 train for London on Friday.'

THE FUTURE AND ITS RELATION TO LIFE

It is sometimes said that the essential characteristic of conscious life is its capacity to store effects of past events and to utilise these to deal with the present situation. This has been called the 'mnemonic function'. I cannot agree that this is either a function peculiar to conscious life, or the function most characteristic of conscious activity. Certainly it is a useful and important function of the conscious organism, but it is one that is shared by many machines, particularly by calculating machines. A more peculiar and more important characteristic of conscious life is the tendency of the behaviour of the organism to be determined by an estimate of what is going to happen next, that is by the future as it appears to the organism. In all our lives, the future or our estimate of it is pressing on us and determining what we are doing now. I am sitting in a train because I have to be in Cambridge in time for supper an hour hence. I am typing an article so that I may send it to the editor tomorrow. Every minute of the day, our activity is directed not primarily by the past or by present needs but by the future. The mnemonic function is useful principally because it serves the ends of the future. I should like to use for this domina-

tion of activity by the future a term parallel to and contrasting with the term 'mnemic function' and to call it the 'promethic function'. Instead of talking of 'precognition', I should prefer to use the term 'promethic *psi* function'.

This carries further the suggestion made some years ago by Wiesner and myself¹ that the use of the terms 'telepathy', 'clairvoyance' etc., suggesting a variety of different paranormal capacities, should be abandoned and that we should instead speak of one paranormal capacity or *psi* function which operates under various experimental or spontaneous conditions. This departure from the traditional system of nomenclature seemed justified by the curious fact, early established by Rhine, that differences in conditions of experimenting at one time regarded as requiring different paranormal capacities seemed to make no essential differences in the subjects' scores². Whether someone else knew the fact to be responded to (in so-called telepathy experiments) or if no one else knows the fact (as in clairvoyance experiments) or if the fact is not yet determined (as in precognition experiments), the average score of subjects remains about the same. For particular subjects at particular times there may be differences, but there seems to be no general difference in the difficulty of performing these apparently different tasks. This suggests that they are not really different tasks but the same task carried out under different conditions. If that is so, our thought is more likely to be correctly guided if we give them the same name. The term 'promethic *psi* function' is not intended to imply that this is a different *psi* function but that *psi* may determine correspondence between behaviour and event under promethic conditions, i.e. when the event lies in the future. The correspondence under *psi* conditions does not show that limitation in time relations which is characteristic of sensori-perceptually determined behaviour. It is sometimes said that *psi* is independent of time. This, however, has not been proved. All that we know about the matter is that *psi* is relatively independent of time as compared with sensori-perceptually determined behaviour.

THE FORMAL CHARACTER OF A PRECOGNITION EXPERIMENT

It is a useful first step in clarifying one's ideas about paranormal phenomena to try to specify as exactly as possible what has really

¹ R. H. Thouless, 'The Present Position of Experimental Research into Telepathy and Related Phenomena', *Proc. S.P.R.*, vol. 47, pt. 166 (1942), pp. 1-19.

² J. B. Rhine, *Extra-Sensory Perception*, Boston, Humphries, 1934.

been observed in a parapsychological experiment, instead of using such non-operational explanatory phrases as 'reading the experimenter's mind', 'exerting a force on the dice', or 'foretelling the future'. Let us then consider exactly what happens in a so-called precognition experiment. There is a series of events *A* and a later series of events *B*, and we observe a correspondence between the *A* series and the *B* series although, in a properly designed experiment, there is no normal causal connection between the two series of events. In this formal outline of the experiment, it does not differ from a PK experiment; the difference lies in the character of *A* and *B* in each of the two cases. Whether any observed correspondence between the *A* series and the *B* series is to be explained by the subject's foreknowledge of series *B* or by his mental influence on series *B*, must depend on whether we can so design the two series that one or other of these explanations is rendered impossible. In a badly designed experiment, such a discrimination may be impossible. In a precognition experiment, the first series is a set of free choices made by the subject. In a PK experiment, it should be a series of target faces determined by some random method, without free choice by either the subject of the experiment or the experimenter. In a precognition experiment, the second series should be one (such as the order of a pack of cards cut at a randomly determined point) which cannot be caused by the volition of the subject or experimenter and which also cannot be produced by them paranormally by psycho-kinesis. In a PK experiment, the second series should be a set of events which cannot be produced normally by the subject, but which might be produced paranormally if psycho-kinesis is a reality.

Many of the early experiments failed to fulfil the conditions necessary for discrimination between precognition and PK. For example, a precognition experiment in which the second series is a set of dice falls would fail to eliminate the possibility that correspondence might be due to PK. Similarly, a PK experiment in which either the subject or the experimenter chooses the target faces fails to eliminate the possibility that correspondence between series *A* and series *B* may be due to precognition of the dice falls and not to any mental influence on their falls.

We may be tempted to ask at this point whether it is possible to design an experiment either in precognition or in psycho-kinesis which completely excludes the possibility of the other explanation. Could we not, for example, suppose that in the precognitive card-guessing experiment, the experimenter (or the subject) was by PK causing the dice to fall with those faces uppermost which would lead him to a number in his table of random numbers which would

lead to a greater than chance correspondence between the already guessed series and the final card order? Obviously it is an unlikely explanation, but is it impossible?

I think the answer is that, if we could assume 100 per cent accuracy in precognition or in PK, then we could always think of an implausible but possible explanation of an apparent precognition experiment in terms of PK and *vice versa*, but that they can be successfully discriminated by a suitably designed experiment if neither is assumed to be completely accurate. For example, in the precognition experiment with cards cut at a random point, three dice may be used, one to determine the page of random numbers to be consulted, one the column, and one the row. Suppose I knew normally or paranormally the order of the pack before the cut and the order of the guesses which the subject has written down and also that there would be better than chance correspondence if the pack was cut at the seventh card which is the number I should get from the tables by dice falls of 2, 4, and 6; obviously, then, if I could paranormally make the dice fall 2, 4, 6, I should secure a better than chance score. But I should not do so if I got a near miss on the dice. If, for example, they fell 2, 4, 5, this would give me a number which might be anywhere from 1 to 25. It might by chance be a number giving me a better than chance score, but it might equally well be one giving me a less than chance score. There would be no ground for expecting from such a near miss anything better on the average than a chance score; the dice must be altogether right if they are to lead to a cut which will consistently give successful results. It is true that if one of the dice is right by PK there is an increased likelihood of the other two being right by chance, and if two are right by PK there is an increased likelihood of the third being right by chance. It follows that a PK efficiency of less than 100 per cent may increase the chances of being right in an experiment designed to test precognition. But even an efficiency of 5 per cent in PK (which is greater than has been reported in most PK experiments) would have an influence altogether negligible in a precognition experiment designed in this way. Even if PK were much more efficient than this, the difficulty could be overcome by making the cut depend on a more complex set of contingent events than the three dice throws of this experiment. We can make it as complex as we like, and every increase of complexity would require a higher efficiency of PK for PK to be a possible explanation of successful results. There is obviously no sensible ground for postulating, as an explanation of success in a precognition experiment, a degree of efficiency of PK incomparably higher than anything we can observe in an experiment designed to test PK.

PRECOGNITION AND DETERMINISM

I do not at all accept the view that experimental (or spontaneous) promethic *psi* implies determinism or any type of theory in which future events are immutably fixed. Let us remember that rational inference enables us to foretell the future with far greater accuracy than any known case of promethic *psi*. So the discovery of promethic *psi* in no way strengthens the argument for a determined future. If promethic *psi* could tell the future with 100 per cent accuracy, there might be grounds for regarding the future as fixed and immutable, but in practice it falls far short of that. Indeed, we can be sure that it must do so, for human volition could always intervene to prevent complete accuracy of prediction. Suppose I were acting as experimenter in a precognition experiment and my subject succeeded in foretelling with complete accuracy the final position of the pack in the first run; and that he then, for the second run, gave me another set of guesses. There is nothing to prevent me from deciding that he shall not be right this second time, so for the new shuffle (or cut) I turn over the cards and deliberately arrange them in an order different from the one he has foretold. Then I should have prevented him from succeeding by my volitional interference with the course of events. It might be objected that he might be still 100 per cent. right in predicting the order the pack would have been in if I had not intervened. Of course he might. The point of this thought experiment is to demonstrate that although he might be successful in foretelling a conditional future, it cannot be certain that he will foretell the actual future. He may succeed in predicting what order the pack would have been in if I had not intervened but not the actual event as determined by my arbitrary intervention. There is also some spontaneous evidence of precognitive dreams in which an event has been foretold which apparently would have happened, but was prevented because the dreamer acted on the warning of the dream.

CAN PRECOGNITION BE EXPLAINED?

Acceptance of the fact of promethic *psi* leads to the question of its explanation. The average scientist, like the man in the street, finds the facts incredible; indeed, he feels that they are such as he could confidently have affirmed would not take place. If asked why he finds promethic *psi* difficult of acceptance, he is likely to give some such answer as: 'It is inconceivable that my behaviour at present can be caused by something which has not yet happened'. It is necessary, however, to ask at this point what is meant by 'inconceivable'. It may refer to a mere psychological

difficulty, to the fact that we find ourselves unable to conceive something; or it may refer to a logical difficulty, to the impossibility of conceiving something because it could not be asserted without a logical contradiction. It is important to remember that we are very prone to mistake a mere psychological difficulty in conceiving for a logical impossibility that the 'inconceivable' proposition can be true.

For example, in the early days of Einstein's special theory of relativity, its critics sometimes asserted that some of its implications were logical absurdities, as, for example, the statement that there was no absolute sense in which it could be asserted that two events taking place at different places were simultaneous. This seemed absurd because it seemed self-evident that if the times of two events were accurately measured it could be discovered whether they took place at the same time or at different times. It was shown, however, that this idea, which seemed a necessity of thought, was really a product of experience. By considering what actually happened when we measured the simultaneity or otherwise of events, in terms of clocks and clock readings, it was shown that simultaneity could not be uniquely determined but that it was a function of the velocity of the frame of reference from which the observations were made. Theoretical physics provides many similar examples which make us distrustful of our intuitive judgment that what seems to us inconceivable must, therefore, be a logical impossibility.

Our conviction that there must be an absolute sense in which we can assert that two events at different places are simultaneous is an example of a proposition whose contradiction seems inconceivable, but in which the difficulty of conceiving rests on no logical impossibility. The propositions of non-Euclidian geometry also are of this type. On the other hand, if I asserted that what happened yesterday could now be made by me not to have happened, I should be asserting something which was inconceivable in the other sense. What is asserted is a logical impossibility, not merely something that is difficult to conceive.

That it must be in the first and not the second sense that promethic *psi* is found to be inconceivable is, of course, established by the experimentally assured fact that it actually happens. What does not happen may not be, in the logical sense, impossible, but we can be quite certain that what does happen is not impossible. The difficulty of conceiving promethic *psi* must, therefore, lie in the limitations of our powers of conceiving. These limitations, moreover, must be superable in theory, however difficult in practice the overcoming of them may be.

It is, indeed, difficult to overcome the intuitive acceptance of

those expectations which are deeply entrenched in our habits of thought and in our customary ways of using language. It is better that we should accept the fact of promethic *psi* without a satisfactory explanation of it rather than that we should too easily accept a bogus explanation. An obviously bogus explanation is that suggested by Einstein's conception that an event *A* anterior to an event *B* when observed within one frame of reference may be posterior to it with respect to another frame of reference. This might suggest that there may perhaps be no absolute sense in which an activity of promethic *psi* is really earlier in time than the event which it foretells. Unfortunately, Einstein's principle only holds for events separated by a space-time interval less than that of the passage of light from the location of one to the location of the other. Both experimental and spontaneous precognitions refer to intervals much greater than this, and for such intervals one event can be unambiguously asserted to be earlier or later in time than another event in a different place. So that line of explanation does not help.

I do not think that we are in a position now to give an explanation, but we can ask what kind of thing an explanation would be. Our first impulse in looking for an explanation is to try to fit the facts of promethic *psi* into the framework of the expectations raised by our present ideas and our present use of language. We should like to make some sort of model in thought which would give the same kind of explanation of determination of behaviour by a future event as we already have for determination of behaviour by past events. That is, for example, what we are doing when we try to imagine (with Dunne) that the future has already in some sense happened in some other dimension of time. We could make a diagram like that, and in the diagram the difficulty of explaining promethic *psi* would not exist. But we should have attained the explanation only by obliterating the essential nature of the future, which is that it has not happened and that its events are to some extent under volitional control.

It seems likely that a more radical re-orientation of thought is necessary before explanation can be reached. If there is a difficulty in explaining promethic *psi*, there is something wrong with our system of expectations. That system of expectations is embodied in the language which is our theory of how things happen in the world. A change in our expectations is needed, and that requires a change in the language embodying our theory of how things happen. When we have the right way of talking and thinking the difficulty will not be merely explained; it will disappear. Promethic *psi* will be seen to be as natural an activity of the organism as perception and memory.

To change a language so that it leads to a more appropriate system of explanations is difficult. Perhaps it always requires a man of genius. Einstein did it for theoretical physics. I sometimes think that if Einstein had been born a little later and if his attention had been engaged by the problems of parapsychology instead of physics, he might have made the new theory into which all the facts of parapsychology would fit. But most of us are not Einsteins and clarification is not likely to be attained by attempts at theorising made by those who have not the rare gift of constructive speculative thought. Nature makes perhaps a thousand competent experimenters for one Einstein, and experimenting is also a road to clarification. When we have far more experimental knowledge about promethic *psi*, the difficulties of making a system of theory in which it will appear a natural and inevitable thing may have been largely cleared away. Either the essential step in thought will have become easy or at any rate the road will have been cleared for a future Einstein of parapsychology. Let us, then, do more experiments.

REVIEWS

PSYCHICAL PHYSICS. By S. W. Tromp. Amsterdam and New York, Elsevier; London, Cleaver-Hulme Press, 1949. xv, 534 pp. 6os.

O. E. Meinzer, introducing an important paper on water divining published in 1917 by the United States Geological Survey, doubted whether so much investigation and discussion has been bestowed on any other subject with such absolute lack of positive results. Professor Tromp, in this weighty tome on divining phenomena, expresses a similar view, and furthermore characterises the publications of most diviners as lacking in all scientific basis, often containing fantastically imaginative assertions concerning facts. But dowisers have upheld their belief for at least seven thousand years, and some of the facts claimed in more than a thousand publications on divining are considered by Tromp to be true. He argues that the basis required for the study of these facts is more important than the examination of the reality of all the alleged facts themselves, and he therefore undertook investigations into what seemed to be certain basic phenomena underlying divining. These researches were carried out in 1946 and 1947 in the physical and physiological laboratories of Leiden University, and in the laboratory of technical physics at

Delft. Professor Tromp defines divining phenomena with some generality as the group of the most complex physico-chemical phenomena occurring in the world surrounding living matter, and unconsciously perceptible by nearly everybody; these phenomena are registered by the nervous system and are transformed by biological amplification into perceptible effects. Using this definition, and adopting the general viewpoint advanced in Professor Tromp's earlier book on Neomaterialism, practically all parapsychological phenomena are seen to be included in divining phenomena. The detailed considerations of Tromp, however, are restricted to dowsing with a rod and radiesthesia, although the distinction between these is not very clear.

Professor Tromp notes certain general difficulties which arise in this field. The great attraction of divining to some people is partly due to an unconscious desire to accept blindly any occult method. Many diviners are obviously charlatans. For practical geophysical prospecting, Tromp (speaking with the authority of a Professor of Geology) states that occult methods are useless, and he recommends dowsers to realise this fact. The honest dowser tends to be offended when his results are doubted, but suggestion can play an enormous role in these extremely subtle observations, and one cannot be critical enough when no instrument can be used in the place of the human body. It is clear that all these factors noted by Tromp seriously complicate experiments with dowsers under field conditions, when even the physical and geological variables are often not well defined. The combination of objective and subjective factors would seem to be such as to facilitate the growth of chimerical theoretical structures on largely erroneous experimental foundations. Professor Tromp has avoided these difficulties by isolating under proper laboratory conditions certain well-defined physical stimuli which he found to produce dowsing reflexes; this approach clearly minimises all disturbing factors except that of suggestion, which was carefully guarded against. The scientific value of the investigations is at the same time increased as these stimuli are identified and simplified, and it is enhanced still further by the precise experimental details given by Tromp and the apparently repeatable nature of his experiments. In these respects the work is in striking contrast to most publications in this field.

Four main basic investigations with dowsers and divining-rods were made. A blindfolded dowser holding a loop-shaped rod could detect a sudden change in a magnetic field of 0.001 oersted, or a magnetic gradient of 0.001 oersted/cm. In a second investigation the change in skin potential of a moving dowser was registered with an Einthoven string galvanometer connected to

electrodes on the two wrists of the dowser. A loop-shaped rod was used, which could rotate freely in two special insulated grips held by the dowser. The loop was connected to one of the electrodes on the dowser's wrist. In these experiments the rod did not turn because of the special grips, but when the dowser entered a dowsing zone, where a normal rod would turn, a change of skin potential, which could amount to 20 mV., was observed. The mere walking of the dowser did not change the level of the Q-peaks in the electro-cardiograms. Similar but less pronounced phenomena were observed with non-dowsers. In a third investigation, local dowsing reactions in houses were related in a striking manner to local magnetic anomalies. In a fourth investigation, small electrostatic charges brought to a divining rod caused a muscular contraction and turning of the rod if the body of the dowser was insulated. The presence of electrostatically charged bodies could also be registered without contact. A weak current through the diviner also produced a reaction. All these experiments are described in detail. To provide a basis for theoretical discussion, the book has a general descriptive section of nearly three hundred pages on the inter-relation of electromagnetic fields and living organisms, and reviews the work of many other investigators of divining, giving about seven hundred references to this topic.

Professor Tromp's summary of the different stimulating forces which, from a theoretical viewpoint, might cause dowsing reactions, reveals a complex picture. The sensitivity of dowsers to electrical stimulation requires a consideration in dowsing experiments of the electrical conductivity and potential gradient in the atmosphere, the conductivity of the soil, earth currents, the electric field of man arising from skin potentials, friction potentials, action currents, brain potentials, and many other phenomena, and perhaps the electric potentials in plants and animals. The sensitivity of dowsers to magnetic gradients requires a consideration of local magnetic anomalies, magnetic variations, and magnetic storms. It may also be necessary to consider, according to Tromp, stimulation by sunlight, ultra-violet light, infra-red rays, cosmic rays, X-rays, acoustic waves, supersonic waves, volatile substances acting on the olfactory cells, and action on the taste cells. Only the electromagnetic, acoustic, and volatile stimulants seem large enough to create a direct excitation of nerve endings in the arm sufficient to stimulate the motor nerves of the arm muscles. A latent period is, however, observed in some experiments between excitation and the appearance of any observable result; the stimulus is therefore considered to be conducted through the thalamus to the different sensorial areas of the cortex. Thus the

main effects of importance for divining above inanimate objects are changes in the dowser's skin potential, changes in atmospheric electrical conditions, variations in the magnetic field of the earth, changes in electromagnetic radiation, and changes in the electrical potential of the dowser's arm due to curvature of equipotential lines in the atmosphere or approach of the dowser to an earthed object. The sensitivity of a dowser may be so great, according to Tromp's experiments, that reactions observed over a person lying on a bed still persist above the bed after the person has left it for hours or even days. This effect is stated to vanish if the bed is earthed sufficiently.

The validity of this picture depends largely on Tromp's basic investigations, which at present do not seem to have been adequately confirmed by other workers. Tromp lists twelve of the main disturbing factors which can cause error in dowsing experiments, such as varying contact between the rod and the palm, leakage of electric charge from the body, variations in speed of the dowser, and in his direction with respect to a zone of disturbance, fatigue phenomena, and varying sensitivity of the reception centre. A great many precautions must be taken in dowsing experiments to minimise all these disturbances. Tromp states various precautions, and recommends the construction of a map showing equi-rhabdomantic lines connecting points where the direction and rate of turning of the divining-rod are the same. The interpretation of this map will present further difficulties. It is clear that one would hardly expect any simple relation between rod reactions and the presence of underground water if a dowser is sensitive to all the stimulants and disturbances enumerated by Professor Tromp. Indeed, nearly all investigations of dowsers under reasonably well-defined conditions have failed to reveal such a relation. If, however, the various complications can be so reduced that the map of equi-rhabdomantic lines corresponds to that of isodynamic lines, which would seem possible from Tromp's experiments, particularly those correlating dowsing reactions with magnetic anomalies, one might expect the greatest success of diviners to be in finding highly magnetic ores such as magnetite and pyrrhotite near to the earth's surface, and divining should be successful with many of the deposits in Sweden. A great many corrections are known to be necessary before the data of a magnetic survey can be computed; Tromp therefore dismisses as unscientific the claims of many dowsers to determine the depth of an ore body by one simple reaction of a rod, and remarks that a great many failures occur in such experiments.

One of the reasons for the possession of dowsing ability is stated to be a low skin resistance. Some dowsers often have a value less

than 50,000 ohms, whereas non-sensitive people have values of 500,000 to 3,000,000 ohms. A number of non-sensitive people could be made sensitive for a short period by washing the palm of their hands with an electrolyte. Thus the findings of Professor Tromp appear to indicate a general effect of magnetic fields on muscular contraction. He summarises a number of experiments by other workers on the effects of magnetic fields on biological systems; usually, however, the effects appeared with rather large magnetic fields, often over 1000 oersted, whereas Tromp obtained his reactions with very small fields. He suggests that small magnetic forces acting for long periods can create deformations similar to those produced by large fields in a short time, and describes some experiments on the growth of mice in a weak magnetic field, which, although far from conclusive, suggest that the field hampers the development of white mice under normal conditions of temperature (for Holland), but has an opposite effect at higher temperatures. Tromp also describes experiments on the influence of magnetic fields and electric charges on the deviation of a pendulum swinging from the hand of a person. Many writers on radiesthesia have assumed that these deviations indicate the presence of new radiations, but no clear evidence for any of these radiations seem to have been advanced. Tromp considers that all the external forces which can influence the divining-rod also affect the pendulum oscillations, but the pendulum is an even more sensitive detector.

Other sections of the book outline the general background of knowledge useful in comprehending Tromp's theories, including interesting accounts of hypnotism, the directional sensitivity of animals, and a brief note on psychical physics, which enumerates some problems worthy of study in the borderland between chemical bipysics and psychical research, and ends with a plea for co-ordinated work by scientists on these topics.

Several incidental suggestions may be noted. Many of the phenomena observed with physical mediums, such as the appearance of lights and the movement of light bodies, may be simply electrical in origin. Experiments by Oppenheim and Koopman on the production of such effects are described. In connection with the stigmata of wounds sometimes observed, an experiment of Gamgee is mentioned, in which he prepared by electrolysis a colloidal form of oxy-haemoglobin which could traverse normally impervious animal membranes.

Whether further work will confirm or contradict the original findings of Professor Tromp remains to be seen; but whatever finally emerges, Professor Tromp has made important advances in concentrating his experiments, on the whole, on simple and

well-defined systems (by the standards of this field of work) and he has given sufficient description of method and details of numerical results to enable any competent worker to repeat the experiments without employing particularly gifted paranormal persons.

A. J. B. ROBERTSON

PSYCHOLOGICAL STUDIES IN TWIN DIFFERENCES. By Gudmund Smith. Lund, C. W. K. Gleerup, 1949. 251 pp. 12.50 cr.

Considerable attention has been given by psychologists in recent years to the study of eidetic imagery, a phenomenon of vision often found in children: when a picture has been carefully scrutinised for a space of time, it appears again upon a blank screen, like a subjective film, sometimes in great detail and with clear reproduction of colour. The researches of Jaensch, most interesting of all in this connection, led to interesting claims that personality-types could be deduced from the different ways in which these images appeared and disappeared in different cases. As psychological characteristics at a very elementary, near-physiological level, these modes of perceptual behaviour were held to possess a close relation to the build-up of the entire personality, including facial appearance and liability to certain illnesses.

The present book describes very thorough-going work carried out at Lund University, for the purpose of answering the question: 'How do after-images and eidetic images look when seen from an hereditary psychological angle?' That is, how far do they reveal psychological qualities which are born in the individual and not shaped by his response to the surroundings? Similarity in hereditary tendencies may be expected to show at its maximum in pairs of uni-ovular or identical twins (denoted in the book as IT), and to a lesser degree in bi-ovular or fraternal twins (FT).

Accordingly, the twin population of Lund and its surroundings was searched, and all available twins of differing age and sex were asked to undergo tests and answer questions. Would it be possible to detect a smaller extent of 'discordance' between IT pairs than between FT pairs by such methods as questionnaires, statements by their next-of-kin and teachers, the comparison of blood-groups, face-angles, and finger-prints, and the results of a psychological puzzle-test in which a pair had to co-operate? Finally, would this minimum of discordance be evidenced by common possession of the eidetic faculty in members of IT pairs? The conclusions imply that this was so.

Even when adult IT had long been separated from each other and followed different careers, 'each IT pair shows in this cross-

section of time a discordance peculiar to the pair, and never lets itself be thoroughly compared with any other pair'. Frequently the pair proves to have a 'co-operation temperament'; one will assume a certain role complementary to that of the other, so that even their differences follow from their relationship. If *A* has 'a more passive disposition and is quieter and more prudent than her sister' then *B* may be 'more active . . . more of a fighter and happy to give her opinion'. But this contrast applies only as between themselves, and they confront the world from an almost identical viewpoint described at length in the case-summary that is given. Seldom is there a real struggle for power between IT. If *A* 'seems to dominate and represent the pair outside', yet 'in reality he must rather give in to *B* and be a kind of servant to him'. The common ground-structure of character can always be found in IT, though in males the differentiation becomes a little greater with time owing to the assumption of different professions; but in FT this common structure does not necessarily exist.

The imagery experiments were carried out in a dark room, the screen being looked at through binoculars. The after-images were measured to find out how far they conformed to a mathematical law said to relate size and distance. Conformity with this law was not invariable, but where it occurred it seemed to distinguish the more 'open and pliable', less individualistic member of the pair. True eidetic tests were then made with pictures of animals, human beings in gaily coloured clothes, and other scenes interesting to both children and adults. Fifteen per cent of the subjects were judged to be eidetic; these were predominantly children under fifteen, and more commonly girls than boys. Within the IT pairs, both were usually eidetic if one was so.

As a sober scientific study this work appears to be unimpeachable. It contains no imaginative theoretical constructs, like the findings of Jaensch. Its numerous and precise statistical tables, its long German quotations, and its use of terms taken from the 'topological' school of psychology, such as 'relevant region' and 'barrier', render it highly technical and not for the general reader. An occasional imperfection in the English is scarcely surprising in view of the difficulty of the material.

Psychical research would benefit from a general increase of knowledge concerning eidetic faculties, since they prove the existence of a normal visual mechanism for 'seeing what is not there'. This mechanism alone does not account for apparitions and clairvoyant perceptions, which are not mere projections of impressions recently fixated by the eye, but it does seem to account for part of the process which enables these experiences to arise. How interesting it would be, for example, to know how

many of those persons who claim to have had such experiences are demonstrably eidetic!

E. G. M.

WONDEREN DER PARAPSYCHOLOGIE. By G. Zorab. Amsterdam, G. W. Breughel, [1949]. 198 pp. f. 6.90.

This book, by the Secretary of the Dutch Studievereniging voor 'Psychical Research', is a useful compilation for those who, reading Dutch, are anxious to become acquainted with the kind of work undertaken by psychical researchers.

The book opens with a summary of opinions on what constitutes the sphere of parapsychology, and then continues with descriptions and discussions of the main classes of alleged paranormal phenomena. Thus we have such problems as those of telepathy and precognition, where Mr Zorab briefly discusses the facts and the interpretations put on them, coupling his remarks with illustrations drawn from the literature. Passing from the mental phenomena, the author then treats of such matters as telekinesis and similar manifestations, where he displays a laudable caution, being careful not to commit himself too far.

Generally speaking, Mr Zorab has succeeded in what he set out to do, namely, to provide an elementary text-book in which the beginner has at hand a brief survey of a wide subject with just sufficient to enable him to see the trees in the wood. It may be that some will think that the author has been too generous in including some of the almost certainly spurious phenomena, but were he to have left out everything in doubt, the book would have hardly been worth writing. As an introduction to the subject for Dutch readers, the book is to be recommended. It is well arranged, clearly printed, and neatly bound, while the index provides a useful key to the kind of material Mr Zorab has included and omitted.

E. J. DINGWALL

OUR LADY OF FATIMA. By William Thomas Walsh. London, Macmillan, 1949. x, 284 pp. 10s. 6d.

It is fairly well known that from 13 May to 13 October 1917 at Fatima in Portugal three peasant children, Lucia aged nine, Francisco aged eight, and Jacinta aged six, had visions, at monthly intervals, of the Mother of God.

The eldest was severely scolded by her mother when the story of the first vision got about (through Jacinta, who was so much excited by it that she could not, as they had agreed, keep quiet

about it.). All three were repeatedly and separately cross-examined, none too gently, by the ecclesiastical and secular authorities, and all recounted the same incidents with the exception of the fact that the girls heard the Lady speak, but the boy only saw her. On 11 August a violently anti-clerical local official summoned them to the town hall at Ourem for further cross-examination. On the 13th, when another apparition was expected, he kidnapped and carried them off to the local jail. He then said that unless they informed him of the secret the Lady had told them they would be boiled in oil, and had them led out one by one to the imaginary cauldron, the second child being told that the first had been 'fried', and the third that this had happened to both her predecessors. None spoke. The vision was four days late that month. The Lady said on several occasions that on 13 October she would perform a miracle—whose nature was unspecified—to convince unbelievers. On that day an enormous crowd of people assembled, some sympathetic, some the reverse; it included representatives of the Press. By noon, when the event, whatever it was to be, had been expected, nothing had happened: an irritated disappointment was setting in, when the pouring clouds parted and the sun, seen as a disc of silver at which it was bearable to look, was perceived as moving up and down, emitting strange colours by which the landscape and the faces of the onlookers were illuminated, and finally, to the general alarm, swooping towards the earth. These phenomena were observed in towns respectively 8 and 40 kilometres away, where children ran out of school to see what was happening. They were also reported in various newspapers, including the anti-religious *O Seculo*, whose editor had been on the spot. It would be very interesting to have a full translation of his contribution, which appeared on 17 October 1927.

In 1938, when a crimson Aurora Borealis was seen all over Europe, Lucia, the only survivor of the three children, a nun, divulged that during the July 1917 apparition the Lady had prophesied that if 'an unknown light' were seen in the night sky, a worse war than that of 1914-18 would break out, and that Russia would 'scatter her errors through the world, provoking wars and persecutions of the Church'.

Mr Walsh's book, written rather as a stimulant to devotion than as a dossier of the case, presents a tidy narrative without loose ends. The contemporary documents are cited in the original Portuguese, but are not quoted at length verbatim. The facts are arranged in a time sequence seen after the event; and it is not made clear until the last chapter that some of these are taken from the four memoirs written by Lucia in 1936, 1937, and August and December 1941,

of which only parts have been published. Though there is no doubt of her sincerity, the years between the peasant child and the disciplined middle-aged nun must have profoundly conditioned her imagery and her vocabulary, and may possibly have smoothed the rough edges of memory.

A work primarily concerned with the purpose and interpretation of the apparitions neither offers nor attempts to offer a detailed and closely documented history of their occurrence, or of the psychological mechanisms through which they were manifested. Its interest for members of the Society for Psychical Research (whose primary concern is more with the functioning of such mechanisms than with the metaphysical question of the impulse that set them in motion) is limited to its general evidence that the phenomena were indeed observed. For a detailed, factual, carefully documented survey of the case reference should be made to Father C. C. Martindale's essay on the subject in the Spring 1948 issue of the *Dublin Review*.

RENÉE HAYNES

JOURNAL OF PARAPSYCHOLOGY, Vol. 13, No. 4, December 1949.
Durham, N.C., Duke University Press.¹ \$1.25.

The Editorial by Professor Rhine deals with the relation between parapsychology and general psychology. He considers that some part of the difficulty in establishing a tolerant interest in parapsychology amongst psychologists is uncertainty amongst the latter as to the nature of their own field. He hopes that by adopting the orientation towards which parapsychology points, general psychology may become able to adopt a leading position amongst the sciences.

Dr Pratt reports a very important research into change of call with success suggested originally by Soal from his results with Shackleton. The interest of this lies in the fact that it suggests that the subject reacts to success although he is not consciously aware of it. Pratt is cautious in accepting this explanation and considers several possible alternative explanations; he shows, however, that these are not admissible explanations of his own results, although one of them may explain the Shackleton result.

Dr Eisenbud reviews a number of psychiatric contributions to parapsychology.

Dr Thouless reports results of a self-experiment in three *psi* tasks—one of clairvoyance and two of precognition—which show that one task may be scoring negatively while another is scoring

¹ Obtainable in Great Britain from the Cambridge University Press, Bentley House, Euston Road, London, N.W. 1, price 10s. per copy or 40s. per year.

positively. Positive correlations between the tasks suggest that positive, negative, and chance scoring may all be due to varying states of balance of positive and inhibitory forces.

Dr Humphrey shows that by dividing subjects into two groups by combining the scores of an interest inventory with those of a drawing test of expansion-compression, a group of percipients can be divided into two groups showing a significantly different scoring capacity.

There are two minor articles. Mrs Rhine discusses the use of parapsychological case histories as guides to experimental investigation, and Cecil French, a doctor of veterinary science, gives an account of a collie bitch whose alleged extra-sensory feats are not unlike those reported by the late Professor Bechterew in the last number of the *Journal*.

R. H. T.

CORRESPONDENCE

REVIEW OF THE WORK OF DR J. HETTINGER

SIR,—It has been suggested that I add to my recent paper in *Proceedings* (vol. 49, pt. 177) on the work of Hettinger the information that in the experiment described on p. 47 Mr Western was aware, when he made his assessment, that controls were present among the pictures. This fact has no bearing on any of the conclusions stated or suggested in my paper, but it is conceivable that another investigator might wish to use the figures in the table on p. 48 for a different purpose, and that Mr Western's knowledge of the presence of controls might be relevant to that purpose.

Mr Western was not, of course, told which pictures were originals and which controls.

CHRISTOPHER SCOTT

EXPERIMENTS ON FREDERICK MARION

SIR,—May I be allowed to comment on some points raised in Dr Soal's review of Frederick Marion's book *In my Mind's Eye* in connection with experiments by Dr Wiesner and myself on Marion.

First, it is said that a statement was signed by Dr Wiesner, Dr West, and Mrs Goldney to the effect that a member of the Magic Circle had duplicated Marion's feat of picking out a red card from

seven black ones. I have never seen this statement since I learned on inquiry at the S.P.R. rooms that they had not retained a copy. If, however, the statement was in the terms here given, it was certainly not correct. The conjuror picked out the red card from seven black ones taken from a pack which he had handled and examined at a previous session at which I was present. In our experiments with Marion, he was not allowed to handle or examine the cards, except in practice experiments for which a special pack was used and which were not counted as critical experiments for our assessment. The pack in question was only one of many packs used, and Marion showed himself able to succeed with a new pack freshly opened at the time the experiment started. Obviously criticism may be made of these card experiments and we later abandoned the use of cards. Nevertheless, such success as Marion had with cards was under conditions not duplicated by the conjuror.

I have read the article by Gibson to which Dr Soal so strongly objects. It seems to me to contain sound points of criticism which deserve to be taken seriously although it is written in a somewhat derisive style which I consider very inappropriate to scientific discussion. I agree with *Nature* in thinking well of Dr Soal's investigation of Marion. I do not, however, think that it was without defects, or that its negative findings are conclusive.

Dr Soal remarks: 'Only the credulous believe that vaudeville mystery showmen perform their feats by telepathy, and why should Marion prove an exception?' The logic of this argument would preclude all study of unusual individuals. Only the credulous believe that professional photographers have paranormal powers, and why should Basil Shackleton prove an exception? Dr Soal proved that he was, and the same possibility arises in connection with Marion, to be settled also by experiment—not, of course, the question of whether he uses paranormal means on the stage, which would be virtually impossible to establish experimentally, but whether he shows unusual paranormal powers under test conditions. If one is looking out for the exceptionally paranormally gifted individual, it may not perhaps be unreasonable to investigate the stage performer. Even if he uses trickery on the stage, he might have been led to the idea of stage telepathic performance by finding that he had paranormal powers which, however, were not reliable enough to depend on by themselves for his effects. Probably one would often be disappointed, but no harm is done by looking for paranormal gifts everywhere they may be found. Whether a particular individual is paranormally gifted is to be determined by experiment and not by argument.

There is an unfortunate lack of detail in Dr Soal's account of his experiments on Marion with Zener cards. He says that Marion showed striking initial success but that his rectangles and circles looked so much alike that Marion had to be consulted during the check as to which was meant. But were these initial successes entirely due to scoring on these doubtful symbols? If so, it would have been easy for Dr Soal to say so ; if not, the experiments with Zener cards were not without evidence for Marion's paranormal capacities.

R. H. THOULESS

SIR.—In addition to the points raised by Dr Thouless, the following comments are suggested by Dr Soal's review.

I feel that other readers will agree with me when I say that it is unusual and perhaps imposes too great a strain upon human frailty to select as reviewer someone who has been criticised in the book under review.

Dr Soal persists in an entirely unwarranted interpretation of his own experiments with Marion. He showed in these experiments not that positive results were inevitably referable to identified or demonstrable sensory clues, but that in certain complex situations positive results tended to disappear. There was no evidence for the operation of sensory clues in many of his experiments, and in the nature of things there could not be any evidence against the operation of *psi*. It is regrettable that Dr Soal, by obscuring this obvious fact, should revive confusion with such assertive emphasis and defective logic.

It must be said, however, in Dr Soal's defence that he himself cannot have been entirely satisfied with his own conclusions ; for he resumed experimental work with Marion long after the publication of his first report which was so much admired by journals devoted to the study of ' normal ' phenomena.

B. P. WIESNER



